The Effect of Compulsory Schooling Laws on Teenage Marriage and Births in Turkey

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Abstract

This paper estimates the impact of the extension of compulsory schooling in Turkey from 5 to 8 years—which increased the 8th grade completion rate for women by 30 percentage points—on marriage and birth outcomes of teenage women in Turkey. We find that increased compulsory schooling years reduce the probability of teenage marriage and births for women substantially, and these effects persist well beyond the new compulsory schooling years: the probability of marriage by age 18 falls by more than 4 percentage points and the probability of giving birth by age 19 falls by more than 4.5 percentage points for the earliest cohorts affected by the policy. In addition, the new policy increases the time to first-birth after marriage. We find conclusive evidence that longer compulsory schooling years have human capital effects on the time to first-birth, as well as incarcertation effects on teenage marriage; there is also suggestive evidence for human capital effects on teenage marriage.

JEL classification: J12, J13, I21, I28, D10.

Keywords: Teenage marriage, Teenage births, Education, Compulsory Schooling Policy, Regression-Discontunity.

1. Introduction

Teenage marriage and births remain at significant levels in Turkey. According to the 2008 Turkish Demographic and Health Survey, of the 25- to 49-year-old women, 43 percent were married before age 20, a quarter were married before age 18, and five percent were married before age 15. The fraction who gives their first-birth at teenage years is also high: of the 25- to 49-year-old women in 2008, 29 percent gave their first-birth before age 20. Adolescent marriage is also widespread around the world. Among the 20-24 year old women, the fraction that is reported to be married before age 18 was 74.5 in Niger, 66.2 in Bangladesh, 55.9 in Mozambique, 49.2 in Ethiopia, 44.5 in India, and 44.3 in Nicaragua according to the most recent Demographic and Health Survey (DHS) statistics for each country.[1] There is also a strong correlation between child marriage and education in several developing countries.[2] In Turkey, among women aged 25 to 49 in 2008, while the median age of marriage was 18.7 for those with no school degree, it was 24.1 for those with a high school degree or above. However, does this imply that an increase in women's educational attainment decreases teenage marriage and births?

In this study, we examine the effects of an increase in women's educational attainment—through the implementation of a longer compulsory schooling duration policy—on age at marriage, age at first-birth, and the time to first-birth after marriage. For this purpose, we use a major education reform in Turkey that took place in 1997. According to this policy, compulsory schooling was extended from 5 to 8 years in 1997. Due to the length of this extension and the high fraction of students who drop out of school after completing the compulsory level in Turkey, this policy brought about a substantial improvement in educational outcomes.[3] In fact, among girls, 8^{th} grade completion rate increased by 30 percentage points, and 11^{th} grade completion rate (high school graduation) increased by more than 7 percentage points.

[1] Source: International Center for Research on Women.

[2] For instance, in Mozambique, while 60 percent of women with no education are married by age 18, this fraction is 10 percent for women with secondary schooling and less than 1 percent for women with higher education. (Source: International Center for Research on Women)

[3] Kırdar (2009) reports for girls whose mother tongue is Turkish, Kurdish, and Arabic that the drop-out rate after the completion of compulsory schooling was 35 percent, 60 percent, and 48 percent, respectively.

In many developing countries, teenage marriage and births have been associated with several adverse outcomes, including worse mother and child health (see, for instance, Alam [2000] for Bangladesh, Raj et al. [2009, 2010] for India), worse educational outcomes (see, for instance, Lloyd and Mensch [2008] for sub-Saharan Africa), and a higher probability of domestic violence (UNICEF, 2005). There is also some evidence for causal effects of early marriage; for instance, Field and Ambrus (2008) find a positive effect of delayed age at marriage on years of schooling in Bangladesh. In Turkey, for mothers younger than 20, neonatal mortality rate was almost twice as much and perinatal mortality rate was almost three times as much as those for mothers aged 20 to 29 during the period from 1998 to 2008 (2008 DHS). In a study conducted in Eastern Turkey, where teenage marriage is especially high, Edirne et al. (2010) find that teenage mothers have lower education and are more likely to face domestic violence, and their births have a higher risk of preterm delivery and low birth weight.

In socially conservative countries, where giving birth out-of-wedlock is socially condemned and therefore is rare, mandating teens to stay in school for longer years is a potentially important intervention on childbearing because longer schooling delays the time at which girls enter the marriage market. A delay in the entry to the marriage market implies an automatic delay in childbearing in these countries. In fact, an important characteristic of the transition into motherhood of women in Turkey is that almost all are married at the time of birth. The sociology literature reports a rigid sequence of events of completion of education, marriage, and, birth of the first child in other countries as well (Blossfeld and De Rose, 1992; Marini, 1984). A key fact in this sequence of events is that the gap between the age at marriage and first-birth is quite narrow in Turkey. Given this narrow gap between the timing of marriage and first-birth and the fact that marriage and schooling are generally incompatible events, a change in the timing of marriage as a result of increased schooling would directly translate into a change in the timing of early fertility as well.

There is substantial evidence for adverse implications of teenage marriage and childbearing in developed countries, as well. In developed countries, the focus has been on the implications of childbearing as marriage is certainly not a prerequisite for motherhood in these countries. This literature has gone beyond establishing associations and uncovered causal effects of teenage motherhood on certain labor market and health outcomes for women and their children. Klepinger et al. (1999), Chevalier and Viitanen (2003), and Fletcher and Wolfe (2009) find that teenage motherhood reduces schooling, work experience and market

wages.[4] Levine and Painter (2003) as well as Holmlund (2005) also confirm that teen-childbearing reduces education substantially. Webbink et al. (2008) find that teenage motherhood increases smoking and the probability of being overweight. On the other hand, studies that investigate the causal relationship between adolescent fertility and child health outcomes have arrived at mixed results (Rosenzweig and Wolpin, 1995; Wolpin, 1997). Intergenerational effects of early childbearing are also reported. Francesconi (2008) finds that children of teenage mothers have lower educational attainment, lower earnings and greater risks of inactivity and teenage childbearing. Hunt (2006) confirms that teenage mothers are more likely to engage in crime. At the macroeconomic level, İyigün (2000) shows that early childbearing may lead to a development trap with low human capital. There have also been studies that focus on the implications of teenage marriage only; for instance, Dahl (2010) finds that teen marriage increases future poverty, and Le Strat et al. (2011) report that it is associated with a higher incidence of mental illness among women in the US.

The data in this study come from the 2003 and 2008 waves of the Turkish Demographic and Health Survey, which is representative nationally and contains detailed information on transitions to marriage and fertility. The break across the year-of-birth cohorts in terms of the exposure to the policy yields the structure of the data suitable for a regression-discontinuity design in estimating the effects of the compulsory schooling policy. We also conduct a battery of robustness checks, using various specifications, sample restrictions, and falsification checks.

Our results indicate that the extension of compulsory schooling in Turkey reduced the probability of marriage and giving birth for teenage women substantially. Moreover, the impact of the extension of compulsory schooling persists well beyond the new compulsory schooling years. We find that, for the 1989 birth-cohort, the percentage of women ever-married by age 15 drops by 50 percent, and the percentage of women ever-married by age 18 drops by 21 percent (more than 4 percentage points). As a result of the rigid sequence of marriage and childbearing, the fraction of women who give birth by age 19 drops by 4.5 percentage points.

We also investigate the channels through which the new compulsory schooling policy affect teenage marriage and births. One channel is the incarceration effect (also called the institution effect), which takes place because schooling and marriage are incompatible events.

[4] An exception is Hotz et al. (2005), who in fact find positive effects on annual ours of work and earnings.

The second channel is the human capital effect, which refers to the effects of increased education after schooling is completed (through its effects, for instance, on wages or preferences). Since all women are virtually out of school after marriage in Turkey, we can test the existence of a human capital effect on the time to first-birth after marriage. We find evidence for incarceration effects of the new education policy on teenage marriage and births, as well as evidence for human capital effects on the time to first-birth. There is also suggestive evidence for human capital effects of the education policy on teenage marriage.

The outline of the paper is as follows. Section 2 places our study in the relevant literature. Section 3 provides background information on marriage and fertility behavior as well as the education system in Turkey, and Section 4 outlines a conceptual framework for this study. Data and descriptive statistics are given in Section 5, and the identification strategy and etimation are explained in Section 6. Section 7 presents the results, and Section 8 provides a discussion of our key findings. Section 9 concludes the paper.

2. Relevant Literature

There have been a number of both quasi-experimental and experimental studies investigating the causal link between education and marriage as well as fertility.[5] The quasi-experimental literature includes settings both in the developed and developing countries. While Black et al. (2008) and Skirbekk et al. (2004) explore the effects of education policies on marriage and birth outcomes (intention-to-treat effects), as we do, others estimate the effect of education on certain marriage and fertility outcomes using IV methodology.

In terms of marriage outcomes, Lefgren and McIntyre (2006) find that education does not have an effect on whether or not a woman in the U.S. ever gets married; on the other hand, examining the timing of marriage, Breierova and Duflo (2004) and Skirbekk et al. (2004) find that education delays the age at marriage in Indonesia and Sweden, respectively. In terms of fertility outcomes, Amin and Behrman (2011), Breierova and Duflo, and Osili and Long (2007) report that the number of children decreases in education in the U.S., Indonesia, and Nigeria, respectively.[6] In a similar Middle-Eastern context to our study—for Arabs in

[5] There have been also some studies that use structural estimation; for instance, Brein and Lillard (1994) find that education increases the age at marriage in Malaysia.

[6] Fort et al. (2011), on the contrary, find that average number of children per woman increases in education; however, this is a cross-country study that uses changes in compulsory schooling laws in various European countries at different times.

Israel, Lavy and Zablotsky (2011) find that education decreases the total number of live births, but has no effect on age at marriage. Skirbekk et al. and Monstad et al. (2008), for Norway, and Amin and Behrman, for the U.S., report that age at birth is delayed by education. Some studies exclusively focus on teenage fertility; for instance, Black et al. (2008)—for the U.S. and Norway—and Ozier (2011)—for the U.S.—show that teenage pregnancy is reduced by education whereas McCrary and Royer (2011) find no such effect in the US.[7]

Unlike the previous quasi-experimental studies examining marriage outcomes, our study focuses on the effect of education policy on teenage marriage. Our examination of teenage births is most similar to that in Black et al. (2008) in the way that we examine the effect of the policy at each age, separately. In fact, Black et al. also estimate the causal impact of changes in compulsory schooling laws on teenage births. However, there are significant contextual differences in the teenage fertility setting of Black et al.—Norway and the U.S.—and ours. First of all, in Turkey marriage is virtually a necessary condition for teenage fertility (which is explained in more detail in the next section); therefore, unlike Black et al., we study the transition to marriage along with the transition to motherhood. Due to the same reason, the channels through which compulsory schooling affects teenage births are different in our context. In fact, we examine whether any change in the time to first-birth is caused by a change in the time to marriage or in the time until first-birth after marriage (or both).

There has also been some recent work that examines the effects of experimental programs that aim to retain girls in school on teenage marriage and childbearing. For instance, Baird et al. (2010), evaluating a cash transfer program to girls in Malawi, find a fall in teenage marriage and pregnancy one-year after the program for program participants who were out of school at the baseline, but not for school girls.[8] In another study in the same setting, Baird et al. (2011) distinguish the effects of conditional cash transfers (CCT) and unconditional cash transfers (UCT). Again, they find a strong effect of school enrollment on marriage; however, since the CCT policy effect on school enrollment is relatively small, the resulting CCT policy effect on marriage is not detectable. On the other hand, they find a strong negative effect of UCT policy on marriage because of the income effect on the large

[7] Since the instruments that Ozier (2011) and McCrary and Royer (2011) use are different, their estimates for the effect of education are for different subpopulations.

[8] The monthly amount of transfer to households was around 15 percent of average household consumption.

group of school drop-outs. Duflo et al. (2006) evaluate the effect of a program that reduces schooling costs in Kenya by providing free school uniforms conditional on school attendance, along with two other inventions. They find that both teenage marriage and childbearing rates go down. An important difference of our analysis from these experimental studies is the heterogeneity in our program impact estimates: we calculate the program impact at each age separately whereas Baird et al. (2010) and Baird et al. (2011) calculate the average effect of the policy for 12-22 and 13-22 age groups, respectively, and Duflo et al. (2006) calculates it for a single grade cohort in one year.[9]

Another important distinguishing characteristic of our study is the strength of the exogenous variation in schooling due to the long duration of the extension of compulsory schooling in Turkey, three extra years, and the high percentage of students whose behavior is actually affected. The other major compulsory schooling policy used in this literature is the extension of compulsory schooling in Norway by two years in 1959 (see, for instance, Black et al. [2008] and Monstad et al. [2008]). Monstad et al. report that the birth-cohorts who were affected by this policy in Norway and who were roughly 5 years younger than the control group had 0.5 years more education. On the other hand, in our study, the completed years of schooling for women by age 18 increases by about 1 year as a result of the education policy.

Finally, the estimated magnitudes of the policy change in our study are much larger than those in Black et al. (2008), who also examine the effect of a major compulsory schooling policy on teenage births by age. For instance, in one of the largest effect they find, the policy of mandating women to stay in school until age 17 reduces the probability of birth by age 19 by 8.8 percent in the US.[10] In comparison, we find that mandating women in Turkey to stay in school until completing grade 8 (roughly age 14) reduces the probability of giving birth by age 17 by a striking 43 percent. In addition, considering the finding by Baird et al. (2011) that a cash transfer policy in Kenya that is unconditional on school enrollment and equivalent to 10 percent of household consumption reduces teenage pregnancy by 27 percent, the effectiveness of the extension of compulsory schooling in Turkey in decreasing teenage marriage and births becomes even clearer.

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[9] Baird et al. (2011) also provide impact estimates for two separate groups by age: 13-15 and 16-22. However, the results are surprising: in the conditional treatment, while the effect of the policy on school enrollment is stronger for the older age group, the effect of the policy on marriage and pregnancy is weaker.

[10] They find smaller effects for Norway.

3. Background Information on Marriage, Fertility, and Education in Turkey

3.1. Marriage and Fertility

Despite the rise in non-marital unions in the West, marriage as a social institution remains strong in Turkey. Almost all women marry by age 49 and 98 percent give birth at least once; in contrast, divorce is an unlikely event, with its rate estimated at less than 2 percent among 15-49 year-old women (2008 DHS). Hence, it would not be incorrect to say that for an average woman in Turkey marriage is for life. Giving birth out-of-wedlock is even rarer than choosing an alternative living arrangement. According to DHS data, almost all births are to a married woman. Age at first-birth is also closely linked to age at marriage; the lapse of time between marriage and first-birth is on average 1.6 years. The average fertility rate currently stands at 2.16 births per woman (2008 DHS).

Marriage occurs early on in life. The median age at first marriage was 20.8 years among women aged 25-49 in 2008 (DHS data). However, age at marriage and age at first-birth have been increasing in Turkey: according to 2008 DHS data, while the median age of marriage was 19.5 for the 45-49 age group, it was 22.1 for the 25-29 age group; in fact, during the 15-year period from 1993 to 2008, the median age at marriage increased by almost 2 years. Despite the rise in age at marriage over time, the incidence of teenage marriage remains high among younger birth-cohorts: of the women aged 25-29 in 2008, 34 percent were married by age 20 and 17 percent were married by age 18 (DHS data).

As a result of their early age at marriage, Turkish women give their first birth at an early age as well. According to the 2008 DHS data, the median age at first-birth for 25 to 49-year-old women was 22.3. Age at first-birth has also increased over time with the rising age at marriage: in 2008, while the median age at first-birth was 21.0 for women aged 45 to 49, it was 23.9 for women aged 25-29. Nonetheless, the incidence of first-birth during teenage years remains high even among the younger generations: of the 25 to 29 year-old women in 2008, 22 percent had given birth before age 20 and 8 percent before age 18 (DHS data).

The Civil Code prohibits the marriage of young men and women before age 17. However, a marriage could still happen through a religious ceremony before the child comes of age. Indeed, 3.84 percent of 16-year-old women in 2008 were married, for instance, and that could only be possible through a religious ceremony. Most couples have both a religious and a civil marriage; however, the fraction of women who only have a religious marriage is

quite significant. For instance, both in 1993 and in 1998, almost 8 percent of all 15 to 49 year-old women had a religious marriage only; this share dropped to 6 percent in 2003 and to 4 percent in 2008 (DHS data). Among teenage women, the fraction with a religious marriage only remains remarkably high. The fraction of 15- to 19-year-old married women who had an only religious marriage was 33 percent in 1993, this fraction dropped only to 29 percent in 2008 (DHS data).

3.2. New Compulsory Schooling Policy

Before the change in the basic education law, the education system in Turkey consisted of five years of primary, three years of lower secondary and three years of upper secondary schooling. The first tier was compulsory. In 1997, compulsory education was increased from five to eight years by combining the first two tiers. Improving the attendance rate at the lower secondary school level was a long established goal. While the net school enrollment rate before the enactment of the law was 89.4 percent in five-year compulsory schooling, it was 52.8 percent in lower secondary and 38.5 percent in higher secondary schooling (Turkish Statistical Institute, 2012). However, the exact timing of the implementation of the new education policy had to do with the political developments of that time. The secular government that came to power in 1997 wanted to prevent children from enrolling in religious schools at an early age. This policy reform would at least delay this by three years.

The new compulsory schooling law, which was enacted in the summer of 1997, covered children who completed grade 4 or lower at the end of the 1996-97 school year (who did not hold a primary school diploma at the time of the implementation of the policy). Therefore, we would see an increase in enrollment rates during the 1998-99 school year for the first time, when the oldest cohort that is affected by the policy start grade 6. Enrollment data from the Turkish Ministry of Education (2011) show that enrollment rose by 4.50 percent during the 1998-1999 school year, by 5.69 percent during the 1999-2000 school year, and by 4.05 percent during the 2000-2001 school year; whereas the rise in enrollment in the 2001-2002 school year was 0.98 percent and the percent increase in enrollment in the following 10 years averaged 0.5 percent. In other words, as a result of the extension of compulsory schooling by 3 years, enrollment in 3 consecutive academic years rose by much more than the average.

Since students who completed grade four or lower grades at the end of the 1996-1997 school year were bound by the new policy, compulsory schooling was for eight years for all students who started the first grade in September 1993 or later; but it was five years for those who started earlier. Even though not all children start school at the same age in Turkey, we do not have the information on school starting age in our data set. Therefore, we assume that all children start school at age six. Accordingly, children who are born at or after 1987 are affected by the policy.

4. Conceptual Framework

We center our conceptual framework on marriage models, in particular those that explain the timing of marriage, because the timing of first-birth in Turkey is largely determined by the timing of marriage. The prominent theories in this area are the marriage model of Becker (1973, 1991) and the search models of Keeley (1977, 1979) and Oppenheimer (1988).

Becker's theory of marriage market is structured around the specialization hypothesis, according to which marriage occurs because the total welfare of husband and wife at the married state are higher—due to specialization of each partner in activities that they have a comparative advantage, market work for husband and household work for wife—than the sum of their individual welfares at the single status. According to this theory, the gains to specialization decrease with the increasing education of women. However, in our context, both men's and women's education increase; therefore, the effect on the gains to specialization is not obvious. Keeley (1977) uses the main features of Becker's theory within a search model to explain the age at marriage. Within this framework, the age of entry into the marriage market and the duration of search for a partner determine the age at marriage, where the duration of search depends on the gains and costs of searching. For women, a higher level of education could decrease the search costs for a partner (by increasing ability in and, therefore, efficiency of the search process), which would increase the duration of search. Moreover, with the increasing education and, therefore, earnings of younger men, the gains from waiting and searching more increase for women. On the other hand, in Oppenheimer's theory, the critical aspect that determines the age at marriage for both men and women—in a traditional setting where women are not in the labor force—is the time at which the uncertainty surrounding men's career path is resolved.

Examining the earlier work both in the economics and sociology literatures, Brüderl and Diekmann (1997) discuss two main channels through which education influences marriage, within the specialization hypothesis: institution effect and human capital effect. According to the institution effect, schooling and marriage are incompatible events. Thornton et al. (1995) point out three main reasons for this: i) students are not prepared for adult roles, ii) school requires a lot of time, iii) financial independence is needed for marriage. This argument is also consistent with Becker's specialization hypothesis because the gains from specialization cannot be realized until after completing schooling: women would not have sufficient time for home production and men would not have sufficent income. The institution effect is also referred to as the "incarceration effect" later in the economics literature (Black et al., 2008).

According to the human capital effect, even after schooling is completed, increased education has a bearing on marriage choices. A particular channel through which this happens is market work: with increased schooling, the opportunity cost of marriage and raising children increases for women. (This is the channel that is emphasized in the work of Becker and in the economics literature in general.) The sociology literature also points out other channels through which more education could affect marriage choices. Axinn and Barber (2001) discuss "ideational theories", which explain how schooling changes fertility preferences. An example to this is that schooling could increase consumption aspirations and, therefore, increase the costs of childbearing and decrease the demand for children. Similar arguments could be applied to marriage; a longer education period could change the preferences on marriage.

Education affects the timing of first-birth through a number of channels.[11] First, the effect of education on marriage translates into an effect on the timing of first-birth in the rigid sequence of marriage and fertility in Turkey, as well as in several other countries. However, there are additional human capital effects of education on the time to first-birth. First of all, a higher level of education for women increases the opportunity of raising children by increasing the market wage rate for them (Willis, 1973). This would decrease the desired

[11] In his study on low-income countries, Schultz (1994a) estimates that an additional year of schooling for women is associated with a 12 percent decline in total fertility. Glewwe (2002) reviews the literature on the relationship between schooling and marriage and fertility in developing countries.

number of children.[12] In addition, Rosenzweig and Schultz (1989) show that education increases the effective contraceptive use of women. Education could provide better knowledge of contraceptive methods via curricula or develop the ability to acquire information about them; the second channel was formalized by Grossman (1972), for which Glewwe (1999) finds empirical evidence in Morocco. Finally, as claimed by Mason (1986), higher bargaining power in fertility decisions for more educated women is another possible channel through which education influences fertility decisions.

5. Data and Descriptive Statistics

The data we employ come from the 2003 and 2008 rounds of the Demographic and Health Survey (DHS) of Hacettepe University of Turkey, which is representative nationally.[13] The target population in DHS surveys are women at the reproductive age; however, information on family members of these women are also available. The surveys include information on the timing of marriage and timing of first-birth on ever-married women, as well as information on schooling attainment in the form of highest grade completed for both women and men. These three pieces of information are our key outcome variables in this study. The DHS surveys also include a rich set of individual and household-level characteristics. In particular, we use information on location of residence at age 12—in the form of both type of location of residence (large city, small city, village) and geographical region of residence (West, South, Central, North, East), mother tongue (Turkish, Kurdish, Arabic), and mother's educational attainment.

We restrict the sample to 10-49 year-old women at the time of each survey; in this case, our pooled sample includes 28,063 women who are born between 1954 and 1998. However, in most analyses, we further restrict the sample to women who are born between 1964 and 1998 (for reasons that will be discussed in the next section), in which case our sample includes 24,619 women. The male sample, which we use solely to examine the effect of the compulsory schooling law on schooling attainment, comes from the 2008 sample only

[12] This is on condition that the substitution effect dominates the income effect (higher demand for children due to a higher level of income). The income effect would be stronger if there is positive assortative mating; in other words, women who acquire more schooling marry more educated men with higher income. Behrman and Rosenzweig (2002) find evidence for assortative mating in the US.

[13] We choose to pool the two rounds of the Turkish DHS to increase the precision of our estimates. Only these two rounds of DHS include birth cohorts that are affected by the new education policy.

because the information on location of residence at age 12 is not available for men in the 2003 survey. The male sample that is retricted to 1964 to 1998 birth-cohorts includes 12,796 persons.[14]

In the first part of our analysis of marriage and birth outcomes—where we examine the effect of the policy on the level of marriage and first-birth outcomes by age, using the information on age-at-marriage and age-at-first-birth, we construct histories of ever-married status and ever-given-birth status until age 21 (or until the age of the woman at the time of the survey if she is younger than 21). [15] This starts at age 10 in the ever-married status analysis and at age 12 in the ever-given-birth analysis, in accordance with the earliest ages these events are observed in the data. Therefore, the women in the sample enter the analysis at each age until 21 unless they are younger than 21 at the time of the survey.

In the second part of our analysis of marriage and birth outcomes, where we examine the effects of the policy on the timing of marriage and timing of first-birth using duration analysis, we construct event histories of time to marriage and time to first-birth. In this case, women enter the risk set of marriage and the risk set of first-birth also at ages 10 and 12, respectively; however, they exit the risk set (and the data) when they get married in the first set and give their first-birth in the second set (unlike the data used in the level analyis). For some women, the duration is censored in the right because they do not marry/give birth until age 21 or until the last age they are observed in the sample if they are younger than 21 at the time of the survey. Unlike the level-analysis, where marriage and first-birth outcomes are examined at each age separately, the data are pooled over ages in the timing analyis. There is a total of 276,329 person-age observations with the 1954-1998 birth-cohort sample, which reduces to 235,001 in the 1964-1998 birth-cohort sample.

5.1. Descriptive Statistics

Here, we provide descritive statistics for our primary sample of women born between 1964 and 1998. Table 1 lists basic descriptive statistics on person-level characteristics in panel (a) and on person-age level characteristics in panel (b). Sampling weights are used in

[14] Later birth-cohorts provide information at earlier ages only; for instance, the 1998 birth-cohort provide information at age 10 only, the 1993 birth-cohort provide information until age 15, whereas all cohorts born at or before 1988 provide information until age 20.

[15] The event history goes until age 21 because this is the latest age we observe the earliest birth-cohort that is affected by the policy, 1987 birth-cohort, in the pooled sample.

the calculation of mean values. Roughly 40 percent of the population in our study live in rural areas. In terms of mother tongue, almost 17 percent are Kurdish speakers and just over 2 percent are Arabic speakers. The low levels of mother's educational attainment is striking: 46.2 percent of mothers have no schooling, and 55 percent have no schooling degree. As can be seen in panel (b), the mean age in our pooled sample is about 15.

< TABLE 1 HERE >

Table 2 reports the fractions of ever-married women and women who have given birth by age in our sample. The fraction of women who are married at quite young ages is non-negligible: by age 14, more than 3 percent are already married. This fraction becomes especially noticeable after age 15. At age 16, 12.4 percent of women are already married. This increases to 29.1 percent at age 18. Accordingly, the fraction of mother teens are also high: almost 4.5 percent of 16-year-old women have given birth, and 23.3 percent have given birth by age 19.

< TABLE 2 HERE >

5.1.1. Graphical Analysis of Schooling, Marriage, and First-Birth over the Birth-Cohorts

The effect of the new education policy on schooling outcomes of both men and women are illustrated in Figure 1.[16] There is a substantial increase among both men and women in the fraction that completes 8th grade as a result of the policy. This increase is not very sharp but is distributed over a few years—this is especially apparent in the profile for women—due to three factors: first, since school start-age differs across children, late starters among the 1985 and1986 birth-cohorts would be affected by the policy and early starters among the 1987 birth-cohort would not be affected. Second, due to the potential signaling effects of the policy—realizing that later-born cohorts will have higher levels of schooling, children born before 1987 could decide to stay in school for a longer time—some of the earlier cohorts would be indirectly affected by the policy.[17] Third, the implementation of the

[16] In this figure, both 2003 and 2008 surveys are used for both men and women.

[17] In the absence of a signaling effect, we would expect the enrollment in the first year at high school (9th grade) to increase in the 2001-02 school year when the first cohort of 6th graders forced to be at school in the 1998-99 school year reach the 9th grade. However, according to Turkish Ministry of Education (2011), enrollment in the 9th grade increased by 21.4 percent in the 1998-99 school year, by 6.7 percent in the 1999-2000 school year, and by 9.5 percent in the 2000-01 school year; whereas it increased by only 1.9 percent in the 2001-02 school year and by 2.9 percent in the 2002-03 school year.

policy was slow in certain areas; therefore, some children among the 1987 and 1988 birth-cohorts may not be affected by the policy. Yet, clearly, nowhere else in Figure 1 is there such a drastic rise in the 8th grade completion rate as there is at the time of the policy. The more interesting feature of Figure 1, though, is the rise in the fraction of both men and women that completes the 11th grade (high school) with the policy. In other words, the new education policy seems to make an effect on grade completion rates well beyond the new compulsory schooling levels. Another important feature of Figure 1 is the strong time trend in the 8th and 11th grade completion rates before and after the implementation of the policy, which is especially prominent for women.

< FIGURE 1 HERE>

Figure 2a and Figure 2b display the fraction women ever married and the fraction women ever given birth, respectively, over birth-cohorts at selected ages. (Fractional polynomials are used in the construction of fitted lines in these figures.) In Figure 2a, the discontinuity in the fraction ever married at the time of the policy is especially visible at ages 14 and 15. Given the fact that the gap between the time of first-birth and the time of marriage is just over a year, the discontinuities in the fitted polynomials in Figure 2b are consistent with the discontinuities in the fitted polynomials in Figure 2a. There are also strong time trends both in the fraction ever married and in the fraction ever given birth. Therefore, in the identification strategy, explained in the next section, it will be critical to disentangle the effect of the policy from this secular time trend in marriage and birth outcomes.

< FIGURE 2 HERE>

6. Identification Strategy and Estimation

6.1. Identification Strategy

We use the variation in the years of compulsory schooling across different birth cohorts to identify the causal impact of the new compulsory schooling policy on marriage and first-birth decisions. The structure of our data, illustrated in Figures 1 and 2, fits a regression-discontiunity design as there is a discontinuous jump in the outcome variables at the time of the policy and the relationship between the outcome variables and the covariate that

determines the timing of the jump (forcing variable) is continuous.[18] Our data-generating process can be written as

$$Y_i = f(x_i) + \rho D_i + \eta_i, \quad (1)$$

where Y denotes the outcome variable, D denotes the treatment variable, and f(.) stands for continuous relationship between the outcome variable and x, the forcing variable (year of birth). In equation (1), ρ is the causal effect of interest and η is the error term.[19] The value of the treatment variable depends on whether or not the forcing variable is above the threshold as follows: $D_i = 1(x_i \geq 1987)$.

A critical aspect of any regression discontinuity design is to distinguish the discontinous jump, $1(x_i \geq 1987)$, from the smooth function f(.). Ideally, one would like to use a very flexible functional form for f(.); however, since the discontinous jump in our data is distributed over a few years, a very flexible f(.) could partly capture the effect of the policy around the discontinuity. However, as can be seen in Figures 1 and 2, the time trends in grade completion, marriage, and first-birth outcomes are all very close to linear. Although we fit a fractional polynomial to the marriage and first-birth outcomes by age in Figure 2, the resulting fits are very close to being linear. Moreover, a more comprehensive analysis by age of the time trends in marriage and first-birth rates, displayed in Figure A1 in the Appendix, confirms that a linear time trend looks appropriate. Therefore, we choose a linear time trend in the most part of our analyses; however, we also check the robustness of our findings using a quadratic time trend.

In all analyses that use a linear time trend, we restrict the sample to 1964 to 1998 birth cohorts—in other words, we exclude 1954 to 1963 birth cohorts—because our graphical analysis shows that our outcomes variables have a more linear trend in this interval. In

[18] Lee and Lemieux (2010) provide a review of regression-discontinuity design. Hahn et al. (2001) construct a theoretical framework for regression-discontunity design, and Angrist and Lavy (1999) and van der Klaauw (2002) are examples to some of the earliest applications. Oreopoulos (2006) uses regression-discontinuity design in his analysis of the effects of compulsory schooling laws on earnings in the UK.

[19] In this study, we estimate the effect of the compulsory schooling policy rather than use it as an instrument for education. The compulsory schooling policy in this study makes substantial changes in both women's and men's distribution of education. This would certainly alter both the marriage market and labor market opportunities significantly for women. As a result, the exclusion restriction assumption—that the policy affects marriage and childbearing outcomes of a woman through the change in her education only—is likely to be violated. Angrist et al. (1996) discuss the factors, like the fraction of compliers with the policy, that would determine the importance of such biases.

addition, since the treatment status of the birth-cohorts right around the discontinuity is not sharp in our data, we use multiple samples in our primary analysis: sample B excludes 1986 and 1987 birth-cohorts, and sample C excludes 1985 to 1988 birth-cohorts, whereas sample A does not make such an exclusion. We also define a sample D, which includes all birth-cohorts from 1954 to 1998 except for those from 1985 to 1988. However, the specifications that are estimated with sample D always include a quadratic-time trend.

If the timing of the change in the education policy were correlated with some unobserved characteristics that also affect marriage and fertility decisions, we would get biased estimates. For instance, if the policy change came right after some shock that decreased school enrollment rates while increasing marriage and fertility, there would be a problem. In this sense, it is important to note that the timing of the policy had to do with the political circumstances in 1997. As explained in Section 2, although improving the low lower-secondary school enrollment rates had long been in discussion by policy-makers, the extension of compulsory schooling was implemented in 1997 because the secular government that had recently came to power saw the policy also as a way of preventing young children from attending religious schools.

6.2. Estimation

6.2.1. Effect of the Policy on the Level of Schooling, Marriage, and First-Birth by Age

In the estimation of the level effects of the policy, we also add a number of control variables to equation (1); therefore, the equation that we estimate is given by

$$Y_i = f(x_i) + \rho D_i + Z_i \beta + \eta_i , \qquad (2)$$

where Z_i denotes the value of covariates for person i. These covariates include mother tongue, location of residence at age 12 in the form of location type (large city, small city, village) and geographical region, and mother's educational attainment. Equation (2) is estimated at each age separately, using a logit regression. Standard errors are clustered at the level of birth-year because the policy variable does not exhibit variation across individuals within a birth-year cohort.

6.2.2. Effect of the Policy on the Timing of Marriage, First-Birth, and First-Birth after Marriage

We use duration analysis to examine the time to marriage, first-birth, and first-birth after marriage. Here, the analysis uses data from different ages within the same estimation procedure; therefore, a time index (for age) is introduced. We choose a logistic form for the hazard function (for marriage and first-birth), given by

$$\log\left[\frac{h_{it}}{1-h_{it}}\right] = b(t) + f_t(x_i) + \rho_t D_i + Z_{it}\beta_t, \quad (3)$$

where t denotes the waiting time concept—which is age, h_{it} is the discrete time hazard rate at time t, b(t) is the baseline hazard rate at time t. The baseline hazard function we choose is non-parametric: a piece-wise constant baseline hazard is used; therefore, we have age dummies for ages 10 to 21. The parameters of the functional form relationship between the forcing variable and the log odds of hazard ratio as well as the impact of the education policy (ρ_t) vary by age. Finally, the effects of other covariates, β_t, are also allowed to vary by age.

Some simplifications in the exact empirical specification of equation (3) are made. We allow the effect of the policy to change by age groups, rather than at each age; however, the time trend, as well as the baseline hazard, is allowed to vary by each age. While the effects of mother-tongue dummy variables and mother's years of education variable are allowed to vary by age, the effect of controls for location of residence at age 12 do not vary by age. However, we use a finer level of controls, compared to Section 4.2.1, by including dummies for the interaction of type of location of residence (large city, small city, rural) with 12 NUTS-1 level region dummies (35 dummy variables).

7. Results

In this section, we first examine the effects of the education policy on schooling outcomes of men and women, which bring about the changes in marriage and birth outcomes. Then, we examine the effect of policy on being ever-married and ever-giving-birth by age for teenage women. Finally, we explore the effect of the policy on the time to marriage and time to first-birth, as well as on the time to first-birth after marriage—which provides us important clues about the channels through which the education policy influences childbearing.

7.1. Impact of the Education Policy on Schooling

Table 3 presents estimation results on the effect of the new education policy on grade completion status for women and men, separately. As explained before, three different samples are used in the estimations that employ linear time trends. In all samples (A, B, and C), there is strong evidence, statistically significant at the 1 percent level, that the new policy increases the completion probabilities of grades 6 to 8—the new years of compulsory schooling—for both men and women. As expected, in sample C, where transition years are excluded, the coefficients are larger: the policy increases the odds of completing the 8^{th} grade by a factor of 5.6 for women and by a factor of 6.3 for men. Moreover, as suggested by the graphical illustration in Figure 1, the policy in fact increases the grade completion rates during high school years, which are not compulsory, for both women and men. Using sample C, there is evidence, statistically significant at the 1 percent level, that the policy increases the odds of the 11^{th} grade completion rate by 34.5percent for women and by 56.1 percent for men.

< TABLE 3 HERE >

In order to assess the magnitude of the policy on schooling outcomes better, we present in Table 4 the predicted grade completion probabilities for the 1989 birth cohort with and without the policy in effect. These predicted probabilities are calculated based on the estimates for sample C in Table 3. The policy increases the 8^{th} grade completion proabability by roughly 30 percentage points for women, from 59.3 to 89.2 percent, and by 23.5 percentage points for men, from 70.3 to 93.8 percent. The rise in the 11^{th} grade completion proabilites are also remarkable: it increases by 7.3 percentage points for women, from 48.3 to 55.6 percent, and by more than 10 percentage points for men, from 54.1 to 64.8 percent.[20]

< TABLE 4 HERE >

The results presented in Tables 3 and 4 are based on a model where the time trend in schooling outcomes is assumed to be linear. According to the visual presentation in Figure 1, this in fact seems to be a reasonable assumption. Nonetheless, we test the robustness of our findings to this assumption by using a quadratic time trend. Yet, using a quadratic time trend has its own limitations. Since we do not have a sharp discontunity, a quadratic time trend is likely to capture part of the effect of the policy—especially in the samples that do not omit the transition period. The exclusion of the transition years alleviates this problem, but also

[20] Tunalı and Yüret (2008) also report higher high school completion rates as a result of the new education policy.

introduces a new problem in the analysis at higher grade levels because then only few birth-cohorts that are affected by the policy remain. These limitations must be taken into consideration in the interpretation of our findings regarding the effect of the policy on grade completion status, with a quadratic time trend, provided in Table A1 in the Appendix.[21]

As can be seen from Table A1, with a quadratic time trend, the estimated effects of the policy on the completion of grades 6 to 8 are smaller for both women and men, compared to those in Table 3. This is expected as the quadratic time trend captures part of the effect of the policy as explained above.[22] Yet, strong evidence, statistically significant at the 1 percent level, remains for the effect of the policy in increasing the 6^{th} to 8^{th} grade completion rates in all samples. In grade levels 9 to 11, there is still strong evidence, with a quadratic time trend, that the policy increases the completion rates of women in sample B. However, in samples C and D, where more transitions years are excluded, both the statistical significance and the magnitude of the effect of the policy are weaker. At higher grade levels—where few birth cohorts that are affected by the policy are left in the sample with a wider exclusion of transition years in samples C and D, it is hard to disentangle the effect of the policy from a quadratic time trend, and the standard errors grow substantially as can be seen in Table A1.

7.2. Impact of the Education Policy on Marriage and First-Birth of Teenage Women

The estimation results on the effects of the education policy on ever being married and on ever giving birth are presented in panels (a) and (b) of Table 5, respectively. The estimates in Table 5 are given for four different samples, where a linear time trend is used in estimations with samples A to C whereas a quadratic time trend is used in the estimations with sample D. Based on the estimates in Table 5, we calculate the baseline and policy predicted values of the probabilities of ever being married and ever giving birth for the 1989 birth-cohort and present these predicted values in Table 6.

< TABLE 5 HERE >

[21] In Table A1, the results for sample A are not provided because the problem of the quadratic time trend capturing the effect of the policy becomes especially severe when 1986 and 1987 cohorts are not excluded.

[22] In sample C, where more transitions years are excluded compared to sample B, the estimated impact in grades 6 to 8 is larger for both men and women because the quadratic time trend is less likely to capture the effect of the policy when more transition years are excluded.

As can be seen from panel (a) of Table 5, there is evidence, statistically significant at least at the 5 percent level, that the education policy decreases the probability of being ever married until age 16 in sample A. As we exclude the transition birth-cohorts in samples B and C, there is evidence for the negative effect of the policy on marriage up to a higher age. In fact, in sample C, the education policy decreases the odds of marriage by age 18 by roughly 25 percent. (As expected, we find stronger effects as we exclude the transition birht-cohorts.) Our estimates using sample D, which are based on a model with quadratic time trends, confirm that the policy decreases the probability of marriage by age 18. At or after age 19, we find no evidence for an effect of the policy on marriage outcomes.

The magnitude of the effect of the policy on the marriage probability at teenage years is striking, as can be seen in panel (a) of Table 6. The probability of marriage by age 16 decreases by 2.62 percentage points, from 6.85 to 4.23 percent, and the probability of marriage by age 18 falls by 4.21 percentage points, form 19.74 to 15.53 percent. In terms of percentage changes, the effect of the policy is stronger at earlier ages; for instance, while the drop in the probability of marriage by age 14 is 63.5 percent, the drop by age 18 is 21.3 percent. This is expected as the policy increases the enrollment rates particularly in grades 6 to 8, which correspond to roughly ages 12 to 14. However, the effect of the policy on marriage persists at ages well beyond the new compulsory schooling years, partly because the effect of policy on school enrollment also persists well beyond the new compulsory schooling years.

< TABLE 6 HERE >

The estimates of the effect of the education policy on ever giving birth are presented in panel (b) of Table 5. The effect of the policy on giving birth by early ages—until age 14—is imprecisely estimated in all samples due to the low frequency of the event at these ages. However, there is strong evidence, statistically significant at least at the 5 percent level, that the policy decreases the probability of giving birth by age 15 and thereafter. The results based on samples A and B reveal that the effect of the policy persists until age 19. In fact, the odds of giving birth by age 19 decreases by about 35 percent as a results of the policy, according to the estimates based on sample B. There is no evidence for an effect of the policy on giving birth by age 20 (which can be tested only using sample A); however, we would need to observe a higher number of cohorts that are affected by the policy to make stronger statements about the effect of the policy beyond the teenage years.

The magnitude of the education policy on birth outcomes is also quite remarkable. According to panel (b) of Table 6, the probabilty of giving birth by age 19 goes down by 4.7

percentage points, from 15.14 to 10.44 percent, for the 1989 birth-cohort as a result of the change in compulsory schooling in Turkey. As it was for the marriage analysis, the percentage drop in the fraction ever giving birth becomes smaller at higher ages. While the percentage drop in fraction ever giving birth by age 15 is 61 percent, it is 31 percent by age 19. Yet, the percentage drop at age 19 is still striking in absolute terms.

The estimated effects of the policy in Table 5 generally become larger as transition cohorts are excluded from sample A to B and from sample B to C, as expected. The estimates based on sample D—where a quadratic time trend is used—are in general less precise; however, statistical significance holds at later ages when the incidence of the event is higher, and the magnitude of the estimated coefficients are similar to those estimated with a linear time trend. In addition, that we find an effect of the policy on marriage by age 18 and on birth by age 19 is also consistent with the earlier discussion that marriage and first-birth can be seen as part of a rigid sequence of events, where the latter is observed soon after the former.

7.2.1. A Falsification Test

Here, we test the effect of an education policy that did not take place, to check the robustness of our findings. For this purpose, we slide the time-frame of our analysis 10 years back: we restrict the sample to cohorts born after 1953 (instead of 1963 as it was in the actual analysis), and we assume that the same education policy was implemented in 1987 (instead of the actual 1997) and, therefore, the policy affects cohorts born in 1977 and afterwards. We also exclude cohorts born after 1984 to make sure that this sample does not include any birth cohorts that might be affected by the policy. (Late starters among the 1985 and 1986 birth-cohorts would be affected by the policy.) Using this sample, we carry out the same empirical analysis in the previous section. In Table 7, the results of this analysis are compared to the results of the actual policy presented earlier (in Table 5). Note that the sizes of the falsification and actual samples are quite similar.

< TABLE 7 HERE >

Table 7 shows that even though the coefficients in the falsification sample are mostly negative, they are substantially smaller than the estimates with the actual sample. In fact, the coefficients in the marriage regressions are very close to zero with the falsification sample. Moreover, the coefficient estimates in the falsification sample are all statistically insignificant, except for that for the first-birth regression at age 16 where statistical significance is at the 10 percent level; however, this coefficent estimate at -0.247 is substantially smaller than the corresponding coefficient estimate in the regression with the

actual sample at -0.637. In essence, we do not find an effect of the education policy with the falsification sample as it is supposed to be with a valid identification method.

7.2.2. Discontinuity Samples

Here, we restrict our analysis to samples covering only the birth-cohorts right around the discontinuity—what Angrist and Lavy (1999) call a "discontinuity sample" in their seminal application of regression discontinuity design. By limiting the sample to a narrow time-band, we can estimate the effect of the policy without polynomial controls for a time effect. The results of this analysis are presented in Table A2 in the Appendix for two different discontinuity samples. In both of these samples, the time frame is restricted to 1983 to 1990. In samples (A1) for marriage analysis and (B1) for first-birth analysis, 1986 and 1987 birth-cohorts are excluded, as it was in Sample B of Table 5; whereas, in samples (A2) for marriage analysis and (B2) for first-birth analysis, 1985 to 1988 birth-cohorts are omitted, as it was in Sample C of Table 5. As can be seen from Table A1, the estimates with the discontinuity samples confirm our findings: the odds ratios in panels (A1) and (B1) for marriage and first-birth, respectively, are very similar to the corresponding values in Table 5 given for sample B. Similarly, there is a very good match between the odds ratios given in panels (A2) and (B2) of Table A1 and the odds ratios for sample C in Table 5.

7.2.3. Other Issues: Different Birth-Cohort Intervals, Time Trends

In a final robustness exercise, we estimate equation equation (2) using different birth-cohort intervals in our sample. Table A3 in the Appendix presents the results for three different samples; two of which cover a wider time-interval of birth-cohorts (after 1953 and after 1958), and one of which covers a narrower time-interval (after 1968). As can be seen from the table, the estimated odds ratios under different samples are very similar to each other, and to those in Table 5. In the last sample—which contains fewer observations—statistical significance is lost at a few late ages due to larger standard errors; however, the magnitude of the odds ratios are similar.[23]

[23] We could not restrict the sample to even narrower time-intervals of birth-cohorts because, with a smaller sample, it becomes hard to statistically disentangle the effect of the policy from the time trend (as already indicated by the last sample in Table A4). However, narrow time-intervals are already examined in Section 6.2.2..

The estimated coefficients for the linear time trends in samples A, B, and C are displayed in Table A4 in the Appendix. In both ever-married and ever-birth analyses, the estimated time trends are stronger at earlier ages, as it was suggested in Figure A1. A comparison of the estimated odds ratios for the time trends with the odds ratios for the policy effect allows us to calculate how many years it would take—in the absence of the policy—for ever-married and ever-birth rates to go down by a level that is equal to that generated by the policy. According to the estimates with sample C, it would take 20 years for marriage by age 14, and 14 years for marriage by age 17 to decrease by a level that is equal to that caused by the policy.

7.3. Impact of the Eduction Policy on the Timing of Marriage, First-Birth, and First-Birth after Marriage

The previous section examined the effect of the education policy on the level of marriage and first-birth outcomes by age. In this section, using duration analysis, we first focus on the effect of the policy on the timing of marriage and first-birth outcomes. Then, we investigate the effect of the policy on the time to first-birth after marriage.

7.3.1. Impact of the Policy on the Time to Marriage and First-Birth

The analysis in Section 5.2 reveals the cumulative effect of the policy on marriage and first-birth outcomes by age; for instance, the effect of the policy on marriage by age 18, reported in Table 5, depends on the effect of the policy on marriage at each age before 18. Even if the policy has no effect on marriage at age 18, the policy may have an effect on marriage by age 18. Duration analysis allows us to uncover the ages at which the policy has an impact on marriage and first-birth outcomes. Moreover, it also allows us to compare the magnitude of the effect of the policy at various ages.

< TABLE 8 HERE >

Panel (a) of Table 8 shows that there is evidence, in all samples, for an effect of the policy on marriage probability at all age groups considered (10-11, 12-14, and 15-17). This effect is much stronger at earlier ages: the policy decreases the odds of marriage by 92 percent at ages 10-11, by 66 percent at ages 12-14, and by 28 percent at ages 15-17 according to sample B. As can be seen in panel (b) of Table 8, the policy decreases the probability of first-birth at ages 15-17 and 18-19 (except for that in sample A, where the effect is weaker due to the inclusion of transition cohorts). In other words, the effect of the policy on first-

birth by age 19, that we illustrated in Table 5, is not only due to the persistence of the effect of the policy at earlier ages—there is in fact a policy effect at ages 18 and 19, which are well beyond the new compulsory schooling years. The magnitude of the policy effect on first-birth is also stronger at earlier ages. The effects at the early ages of 12 to14 are very imprecisely estimated, as it was in Table 5. All these findings for both marrige and first-birth are robust to the inclusion of quadratic time trends in sample D.

7.3.2. Impact of the Policy on the Time to First-Birth after Marriage

The fall in the fraction of women who give birth at young ages, illustrated in Section 5.2, could arise from two different mechanisms. First, it could be brought about by a change in the age at marriage; and, in fact, we illustrated in Section 5.2 that the fraction of women who get married at young ages went down as a result of the policy. However, there is another mechanism through which the fraction of women who give birth at young ages could go down. As a result of the higher education levels, caused by the education policy, married women could be delaying the birth of their first-child. In this subsection, we tackle this question: after a woman is married, do the longer compulsory schooling years increase the time to first-birth?

In this analysis, women enter the risk set once they are married. The estimation results are presented in panel (c) of Table 8. In samples A, B, and C, there is evidence (statistically significant at least at the 5 percent level in samples A and B) that the education policy decreases the odds of first-birth at ages 15-17 and 18-19 for married women. Using sample D and quadratic time trends, there is statistical evidence at ages 18-19 only; however, the coefficient estimate at ages 15-17 is similar to those estimated with samples A to C. Therefore, we can conclude that the education policy also increased the time until first-birth for married women. This finding is quite important because unlike the time to marriage and time to first-birth analyses, in the time to first-birth after marriage analysis we know for sure that women are out of school (in the rigid sequence of schooling, marriage, and fertility in Turkey). In other words, the education policy has effects on women's birth behavior even after they are out of school. This issue we discuss more in the next section.

8. Discussion

In the conceptual framework section, we discussed two main channels through which education affects marriage and, therefore, birth outcomes: incarceration effect and human capital effect. Here, we interprete our key findings in this framework.

We find a very strong incarceration effect of the education policy on teenage marriage and births. The probability of marriage drops substantially during the ages that girls are mandated to stay in school with the new education policy. At the same time, we also find that the effect of the new education policy on teenage marriage and births persists well beyond the new compulsory schooling years. While most children would complete grade 8 by age 14 or 15, we find that the effect of the policy on marriage persists until age 18, and on first-birth until age 19. Similarly, Black et al. (2008) find that the effect of compulsory schooling on teenage fertility in the U.S. and in Norway also persists beyond the years the children are forced to stay in school, and interpret this as evidence for human capital effect. However, such a conclusion is not possible in our context because the effect of the new education policy on grade completion grades also persists well beyond the new compulsory schooling years in Turkey. In fact, even high school graduation rates—high school graduation takes place at age 17 or 18 for most people—increase remarkably as a result of the policy.

Nonetheless, the results suggest that a human capital effect of the education policy also exists. If there was only an incarceration effect of the policy on marriage, the women who delay their marriage—that would otherwise happen at grade levels 6 to 8—due to the policy, would get married once the new compulsory schooling years are over. This 'catching-up effect' could easily overwhelm— in the absence of human capital effect—the negative effect of the policy on marriage due to the increased enrollment at high school grades, because the fraction of girls who are induced to complete grades 6 to 8—for whom the 'catching-up effect' would apply— is much larger than the fraction of girls who are induced to complete high school grades.[24]

[24] We illustrate this idea quantitatively as follows. The new compulsory schooling policy induces roughly 75 percent of the girls who would not otherwise complete grades 6 to 8 to complete them (Table 4). Using the marriage rates at the corresponding ages (12 to 14) and assuming that marriage is random among the group of girls who comply with the policy and the group who do not comply with it, we calculate that 2.25 percent of the girls in our sample would delay their marriage (that would otherwise happen at ages 12 to 14). At the same time, the new policy induces roughly 20 percent of the girls who would not otherwise complete grades 9 and 10 to complete them. Using the marriage rates at the corresponding ages (15 and 16) and maintaining the

At the same time, we find clear evidence for a human-capital effect of the policy on the time to first-birth. The fact that a higher education level leads to an increase in the time to first-birth after marriage could obviously not be a result of the incarceration effect of the policy because—in the rigid sequence of schooling, marriage, and fertility in Turkey—all married women are out of school. This implies that increased education either increases the ability of teenage mothers to control their fertility or influences their preferences about it.

Our estimated magnitude of the effect of the compulsory schooling policy on teenage fertility is much larger than that reported in developed countries (Black et al., 2006). In Western societies, teenage fertility may not be necessarily planned; it may happen due to idiosyncratic events. On the other hand, in Turkey, it is clearly planned along with marriage. Therefore, schooling puts a very strong break on teenage fertility in Turkey by preventing marriage, whereas it causes just a deceleration in the U.S. and Norway by decreasing the chances of pregnancy. On the other hand, similarly large effects are reported for developing countries. For instance, Ozier (2011) reports that a policy that brings about a 13 percetage point increase in high school graduation rate in Kenya also causes a 12 percentage point reduction in pregnancy by age 18. (We find a 4.5 percentage point reduction in first-birth by age 19.)

Poverty is often pointed out as an important underlying factor for early marriage in several countries.[25] Within Keeley's (1979) search model, we can think of poverty as increasing search costs, thereby reducing the age at marriage. The new compulsory schooling policy in Turkey decreased the monetary costs of school attendance in grades 6 to 8 because the accessibility of schools increased: the government had to either provide schools that included these grade levels even in sparsely-populated areas or transport the students in these

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assumption of randomness of marriage among compliers and non-compliers, we calculate that 1.87 percent of the girls in our sample would delay their marriage (that would otherwise take place at ages 15 or 16). If there was only an incarcertation effect of the policy, we would expect the girls who would marry in the absence of the policy at ages 12 to14 (2.25 percent) to marry at ages 15 or 16 once the new compulsory schooling years are over. However, their fraction is larger than the fraction that is induced not to the marry due to school enrollment in grades 9 and 10 (1.87 percent). Then, we would not observe a reduction in the marriage rates at high school ages, as we actually do. We realize that this sketch makes strong assumptions, but it is merely to illustrate the idea.

[25] For instance, UNICEF (2005) reports for Senegal that girls in the poorest 20th percentile of households are more than 4 times as likely to be married as girls in the welathiet 20th precentile of households.

areas to a school on a daily basis. This fall in the costs of school attendance would obviously increase the opportunity cost of marriage.

Nonetheless, previous studies on early marriage in Turkey point out cultural factors, rather than economic factors, as the key driving elements. For instance, Edirne et al. (2010) find that the parents of teenage mothers have lower education and are more likely to follow matrimonies, but do not have lower household income than other parents. In their qualitiative study in Eastern Turkey, Ertem and Koçtürk (2008) highlight the importance of "protecting family honor" as a reason for marriage once girls reach the age of menarche. If, in fact, cultural traditions are the main driving factor of early marriage and fertility, this study shows that these cultural traditions are not impossible to change, at least in Turkey.

9. Conclusion

In this paper, we estimate the impact of the extension of compulsory schooling from 5 to 8 years in Turkey on the marriage and fertility decisions of teenage women. We find that the rise in compulsory schooling years indeed reduces the probability of marriage and giving birth for teenage women. Moreover, the magnitude of this effect is quite substantial. The proportion of women married by age 18 drops by more than 4 percentage points, and the proportion of women who give birth by age 19 drops by more than 4.5 percentage points for the 1989 birth-cohort (one of the ealier cohorts affected by the policy).

We find a very strong incarceration effect of the new compulsory schooling policy on marriage and, therefore, on first-birth in Turkey. The probability of marriage is reduced susbstantially during the new compulsory schooling years. The policy decreases the probability of marriage by age 15 by 50 percent for the 1989 birth-cohort. Moreover, the effect of the policy extends well beyond the new compulsory schooling years; in fact, there is evidence that its effect on marriage persists until age 18, and its effect on first-birth persists until age 19. The effect of the policy on marriage and first-birth beyond the new compulsory schooling years could still result from an incacertation effect, as well as a human capital effect, because grade completion rates beyond the new compulsory schooling years also increase as a result of the policy.

The delaying effect of the new education policy on first-birth could be brought about by two different mechanisms: by delaying the timing of marriage or by increasing the time to first-birth after marriage (or both). We find evidence for both. The fact that the new education policy increased the time to first-birth after marriage—a period where there is no

incarcertation effect in Turkey—implies that there are human capital effects of increased schooling.

In essence, this study shows that increasing education at the lower end of the distribution, by an extension of compulsory schooling years, delays teenage marriage and childbearing substantially in an upper-middle income (World Bank classification) developing country. What remains to be seen in the Turkish context is the effect of this policy on total fertility, as well as spacing of births, which can be answered only after the initial cohorts affected by the policy complete their fecund period. Nevertheless, the previous empirical literature shows that increasing the age at marriage and childbearing would have important economic implications. Given the causal links between age at marriage and age at first-birth with several other demographic decisions (like divorce, number and spacing of children) and health outcomes (like maternal and child mortality) as well as economic decisions (labor force participation, migration) and outcomes (welfare take-up), the rise in age at marriage and age at first-birth would have important implications on the aggregate demography and economy, including population growth and economic growth. Moreover, they would have intergenerational impacts through their effects on child health and education. Finally, age at marriage and age at first-birth have non-economic implications on domestic violence and women's decision-making power within the household.

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Figure 1: Fraction Completed Selected Grade Levels by Year of Birth

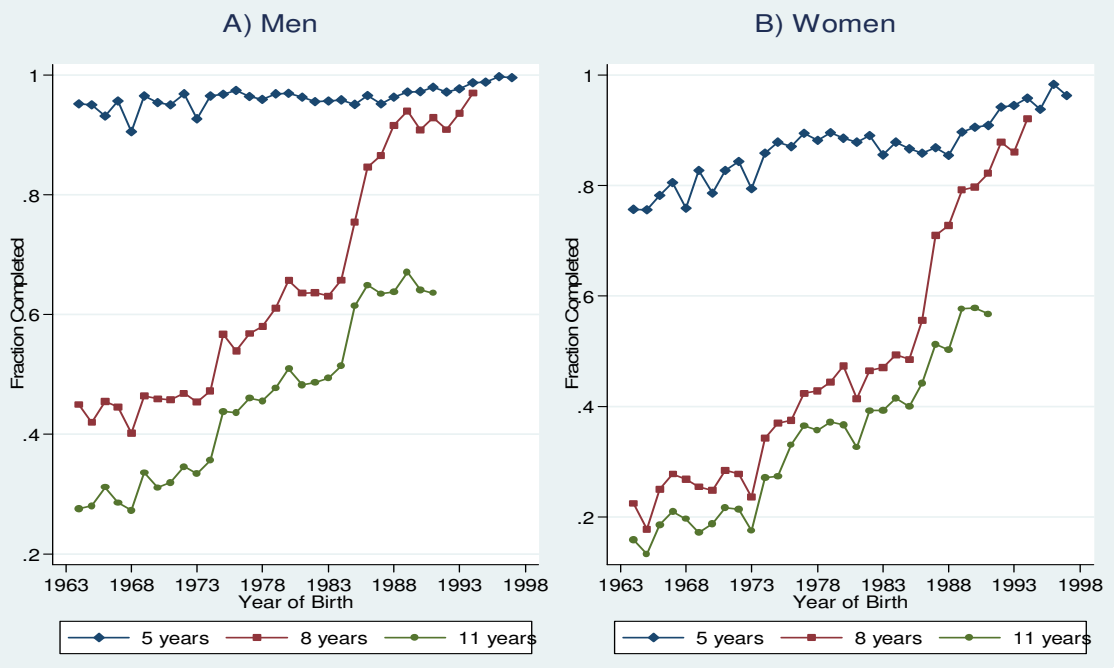
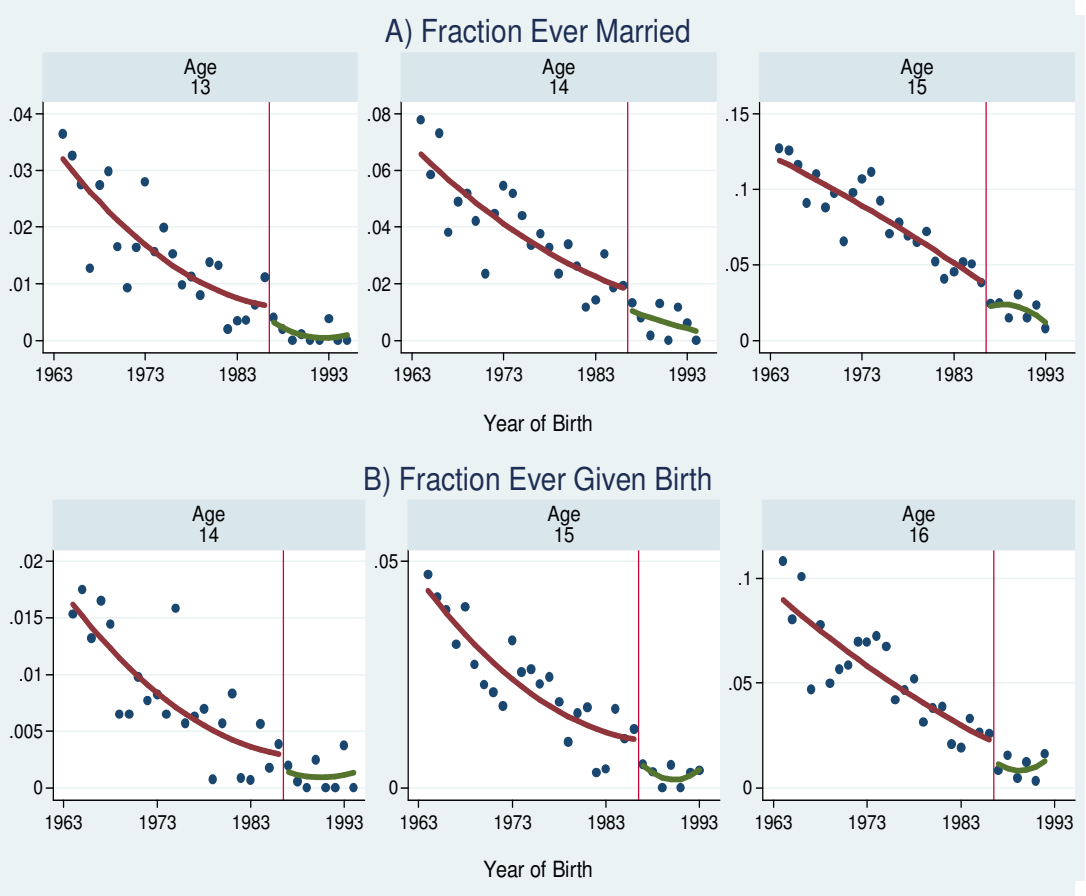


Figure 2: Fractions of Women Ever Married and Women Ever Given Birth at Selected Ages by Year of Birth



Notes: The fitted lines are based on fractional polynomials.

Table 1: Descriptive Statistics for Female Sample

	Mean	S.D.	Min.	Max.	No. Obs.
A) Person Level Characteristics					
Year of Birth	1981.1	9.223	1964	1998	24,619
Type of Place of Residence at Age 12					
Province Center	0.391	0.485	0	1	24,390
Other City/Town	0.203	0.408	0	1	24,390
Village	0.406	0.492	0	1	24,390
Region of Residence at Age 12					
West	0.339	0.414	0	1	24,384
South	0.128	0.342	0	1	24,384
Center	0.154	0.338	0	1	24,384
North	0.136	0.359	0	1	24,384
East	0.243	0.481	0	1	24,384
Mother-Tongue					
Turkish	0.809	0.435	0	1	24,004
Kurdish	0.169	0.419	0	1	24,004
Arabic	0.022	0.159	0	1	24,004
Mother's Educational Attainment					
No School	0.462	0.500	0	1	23,131
Some School but No Degree	0.088	0.279	0	1	23,131
Compulsory Schooling	0.361	0.468	0	1	23,131
More than Compulsory Schooling	0.089	0.270	0	1	23,131
B) Person-Age Level Characteristics					
Age	14.972	3.404	10	21	235,001
Year	1993.5	8.158	1974	2008	235,001
Education Policy	0.187	0.399	0	1	235,001

Notes: West is defined as NUTS1 1 to 4 regions, South as NUTS1-6 region, Center as NUTS1 5 and 7 regions, North as NUTS1 8 and 9 regions, and East as NUTS1 10 to 12 regions. Education policy variable takes the value of 1 for birth-cohorts born in 1987 and afterwards, 0 otherwise.

Table 2: Fractions of Women Ever Married and Women Ever Given Birth

Age	10	11	12	13	14	15	16	17	18	19	20	21
% Married	0.04	0.14	0.42	1.17	3.04	6.72	12.4	20.0	29.1	38.3	47.3	55.6
% Given Birth	--	--	0.06	0.23	0.60	1.84	4.47	8.94	15.2	23.3	32.5	41.1

Table 3: Effect of Education Policy on Grade Completion for Women and Men

A) Women									
	Sample A			Sample B			Sample C		
Grade	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.
1	0.937	0.144	23,792	0.916	0.159	21,960	0.996	0.170	20,159
2	0.877	0.145	23,792	0.882	0.160	21,960	0.953	0.170	20,159
3	0.884	0.153	23,792	0.885	0.162	21,960	0.935	0.189	20,159
4	0.880	0.188	23,792	0.880	0.208	21,960	0.941	0.216	20,159
5	0.844	0.181	22,970	0.822	0.182	21,138	0.862	0.179	19,337
6	5.057***	1.023	22,204	6.462***	1.259	20,372	7.939***	1.342	18,571
7	4.392***	0.756	21,426	5.516***	0.853	19,594	6.522***	0.880	17,793
8	3.853***	0.623	20,608	4.815***	0.716	18,776	5.643***	0.880	16,975
9	1.486***	0.117	19,859	1.628***	0.122	18,027	1.589***	0.159	16,226
10	1.297***	0.076	18,956	1.355***	0.096	17,124	1.380***	0.118	15,781
11	1.248***	0.078	17,999	1.312***	0.090	16,675	1.345***	0.109	15,332
B) Men									
	Sample A			Sample B			Sample C		
Grade	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.
1	1.325	0.311	11,033	1.377	0.368	10,453	1.295	0.374	9,799
2	1.104	0.293	11,033	1.134	0.336	10,453	1.071	0.363	9,799
3	1.231	0.334	11,033	1.245	0.383	10,453	1.235	0.448	9,799
4	1.192	0.378	11,033	1.342	0.484	10,453	1.616	0.615	9,799
5	1.083	0.323	10,591	1.223	0.417	10,011	1.252	0.430	9,357
6	5.160***	1.301	10,157	7.083***	1.433	9,577	8.537***	1.859	8,923
7	4.529***	1.144	9,739	6.350***	1.158	9,159	7.503***	1.403	8,505
8	4.125***	0.929	9,318	5.580***	0.878	8,738	6.349***	1.009	8,084
9	1.595***	0.235	8,916	1.848***	0.249	8,336	2.022***	0.253	7,682
10	1.349**	0.170	8,523	1.533***	0.168	7,943	1.603***	0.174	7,289
11	1.329**	0.151	8,114	1.471***	0.164	7,534	1.561***	0.171	6,880

Notes: The sample includes 1964 to 1998 birth cohorts for 10 years old, 1964 to 1993 birth cohorts for 15 years old, and 1964 to 1988 birth cohorts for 20 years old. However, due to the fuzzy nature of the discontinuity, 1986 and 1987 birth cohorts are omitted in sample B and 1985 to 1988 birth-cohorts are omitted in sample C. A separate regression is run at each school year from 1 to 13 where the dependent variable is completion of school year. In addition to the dummy variable for the education policy, the specifications include a control for a linear time trend as well as controls for mother's mother-tongue (Kurdish, Arabic), and the location of residence at age 12 (in the form of the type of location of residence [large city, small city, rural] and 5 geographical regions [west, south, central, north, east]). The sample for men is based on the 2008 survey only as the 2003 survey do not include information about the type of location of residence for men, whereas the female samples are based on both 2003 and 2008 surveys. Standard errors are clustered at the level of year of birth. *** statistical significance at 1 percent level; ** at 5 percent level; * at 10 percent level.

Table 4: Policy Effect on Predicted Grade Completion Rates for Men and Women

		A) Women				B) Men		
Grade Level		Baseline	Policy			Baseline	Policy	
6		0.604	0.924	***		0.744	0.961	***
7		0.594	0.905	***		0.710	0.948	***
8		0.593	0.892	***		0.703	0.938	***
9		0.500	0.614	***		0.578	0.734	***
10		0.490	0.570	***		0.559	0.670	***
11		0.483	0.556	***		0.541	0.648	***

Notes: The predicted values are for the 1989 birth cohort, based on the estimates for sample C given in Table 3. All other variables are set at their mean values for women of the 1989 birth-cohort in panel (a) and for men of the 1989 birth-cohort in panel (b). The difference between the baseline and policy values is statistically significant at 1 percent level; ** at 5 percent level; * at 10 percent level.

Table 5: Effect of the Education Policy on Ever Being Married and on Ever Giving Birth for Teenage Women

A) Dependent Variable: Ever Married													
	Sample A			Sample B			Sample C			Sample D		
Age	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.
11	0.243	0.212	21,634	0.097**	0.110	19,999	--	--	--	--	--	--
12	0.146***	0.102	20,882	0.060***	0.056	19,247	--	--	--	--	--	--
13	0.307**	0.147	20,112	0.213***	0.111	18,477	0.219**	0.145	16,866	0.326	0.244	20,154
14	0.466**	0.139	19,308	0.372***	0.130	17,673	0.363**	0.171	16,062	0.403	0.236	19,350
15	0.530***	0.067	18,576	0.489***	0.077	16,941	0.489***	0.110	15,330	0.535**	0.143	18,618
16	0.693***	0.092	17,759	0.709**	0.108	16,124	0.601***	0.093	14,926	0.683**	0.129	18,214
17	0.928	0.132	16,889	0.799	0.115	15,715	0.665***	0.092	14,517	0.732**	0.106	17,805
18	0.883	0.090	16,002	0.819**	0.082	15,311	0.748**	0.097	14,113	0.789*	0.105	17,401
19	1.026	0.093	15,124	0.961	0.110	14,433	--	--	--	--	--	--
20	1.067	0.065	14,372	--	--	--	--	--	--	--	--	--

B) Dependent Variable: Ever Birth												
	Sample A			Sample B			Sample C			Sample D		
Age	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.
12	1.247	1.714	20,882	--	--	--	--	--	--	--	--	--
13	0.348	0.257	20,112	0.130**	0.127	18,477	--	--	--	--	--	--
14	0.480	0.235	19,308	0.410	0.267	17,673	0.450	0.342	16,062	0.587	0.553	19,350
15	0.429***	0.119	18,576	0.387***	0.132	16,941	0.372**	0.181	15,330	0.550	0.397	18,618
16	0.472***	0.109	17,759	0.529**	0.146	16,124	0.472**	0.171	14,926	0.638	0.269	18,214
17	0.642**	0.124	16,889	0.558**	0.127	15,715	0.452***	0.127	14,517	0.544**	0.162	17,805
18	0.791**	0.091	16,002	0.752**	0.098	15,311	0.625***	0.068	14,113	0.728**	0.102	17,401
19	0.757**	0.103	15,124	0.653***	0.089	14,433	--	--	--	--	--	--
20	0.968	0.105	14,372	--	--	--	--	--	--	--	--	--

Notes: For each sample, a separate regression is run by age where the dependent variable is ever married status in panel (A) and ever given birth status in panel (B). The original sample (sample A) includes 35 birth-year cohorts, 1964-1998, for 10 years old; 30 birth-year cohorts, 1964-1993, for 15 years old; and, 25 birth-year-cohorts, 1964-1988, for 20 years old. Due to the fuzzy nature of the discontinuity, 1986 and 1987 birth cohorts are omitted in Sample B, and 1985 to 1988 birth cohorts are omitted in Sample C. Sample D covers 1954 to 1998 birth cohorts, excluding 1985 to 1988 birth cohorts. While a linear time trend is included in regressions using samples A to C, a quadratic time trend is included in regressions using sample D. The other control variables include mother's mother-tongue (Kurdish, Arabic), mother's educational attainment, and the location of residence at age 12 (in the form of the type of location of residence [large city, small city, rural] and 5 geographical regions [west, south, central, north, east]). Standard errors are clustered at the level of year of birth. Some cells for age 12 and 13 are missing because the policy effect is not identified due to infrequent incidence of marriage and birth at these ages. Some cells at the latest ages, ages 19 and 20, are missing because the earliest waves of birth cohorts that are affected by the policy are excluded due to the fuzzy nature of the discontinuity. *** statistical significance at 1 percent level; ** at 5 percent level; * at 10 percent level.

Table 6: Effect of Education Policy on Predicted Percentages of Ever Being Married and Ever Giving Birth

A) Ever Married					B) Gave Birth				
	Baseline	Policy		% Drop		Baseline	Policy		% Drop
Age 13	0.21	0.05	**	78.1	Age 13	0.03	0.00	**	87.0
Age 14	1.04	0.38	**	63.5	Age 14	0.10	0.04		59.0
Age 15	3.14	1.56	***	50.3	Age 15	0.45	0.18	***	61.2
Age 16	6.85	4.23	***	38.2	Age 16	1.47	0.78	**	46.8
Age 17	12.47	8.66	***	30.6	Age 17	4.24	2.41	**	43.1
Age 18	19.74	15.53	**	21.3	Age 18	8.45	6.48	**	23.2
Age 19	--	--		--	Age 19	15.14	10.44	***	31.1

Notes: Predicted values are given for the 1989 birth-cohort, where all other variables are set at their mean values for this birth cohort. In calculating the predicted values for ever married, estimates from Sample C in panel (a) of Table 5 are used; and in calculating the predicted values for given birth, estimates from Sample B of panel (b) of Table 5 are used because Sample C does not provide estimates at age 19. That the predicted policy value is different from the predicted baseline value is statistically significance at 1 percent level; ** at 5 percent level; * at 10 percent level.

Table 7: Falsification Test - Effect of an Education Policy that did not Take Place

	Falsification				Actual		
	Coef.	S.E.	No obs		Coef.	S.E.	No obs
A) Ever Married Regressions							
Age 13	-0.354	0.225	16,634		-1.520**	0.663	16,866
Age 14	-0.067	0.164	16,634		-1.013**	0.470	16,062
Age 15	-0.038	0.133	16,634		-0.716***	0.226	15,330
Age 16	-0.091	0.098	16,634		-0.509***	0.154	14,926
Age 17	-0.122	0.086	16,634		-0.407***	0.138	14,517
Age 18	-0.044	0.063	16,634		-0.291**	0.129	14,113
B) First-Birth Regressions							
Age 13	0.154	0.558	16,634		-2.038**	0.977	18,477
Age 14	-0.218	0.352	16,634		-0.891	0.652	17,673
Age 15	-0.240	0.218	16,634		-0.950***	0.341	16,941
Age 16	-0.247*	0.147	16,634		-0.637**	0.276	16,124
Age 17	-0.086	0.149	16,634		-0.583**	0.228	15,715
Age 18	-0.138	0.124	16,634		-0.285**	0.131	15,311
Age 19	-0.113	0.095	16,634		-0.426***	0.137	14,433

Notes: The falsification sample includes 1954 to 1984 birth cohorts, who are not affected by the policy. The actual sample in panel (a) is the same sample as "Sample C" in Table 5 as predictions on ever married status in Table 6 are based on this sample; and, the actual sample in panel (b) is the same sample as "Sample B" in Table 5 as predictions on ever given birth status in Table 6 are based on this sample. For all samples, a separate regression is run by age, where, in addition to the dummy variable for the "artificial" education policy that afects cohorts born in 1977 and afterwards and the actual education policy that affects cohorts born in 1987 and afterwards, the specifications include a control for a linear time trend as well as controls for mother's mother tongue (Kurdish, Arabic), mother's educational attainment, location of residence at age 12 (in the form of type of location of residence [rural, small city, large city] and 5 geographical regions). Standard errors are clustered at the level of year of birth. *** statistical significance at 1 percent level; ** at 5 percent level; * at 10 percent level.

Table 8: Duration Analysis Results - Effect of Education Policy on Time to First Marriage, Time to First Birth, and Time to First Birth after Marriage

A) Time to Marriage								
	Sample A		Sample B		Sample C		Sample D	
	Odds Ratio	S.E.	Odds Ratio	S.E.	Odds Ratio	S.E.	Odds Ratio	S.E.
Policy * Ages 10-11	0.209*	0.182	0.081**	0.091				
Policy * Ages 10-14					0.180**	0.128	0.275*	0.212
Policy * Ages 12-14	0.433***	0.127	0.342***	0.118				
Policy * Ages 15-17	0.778**	0.084	0.725***	0.086	0.632***	0.084	0.732**	0.112
Policy * Ages 18-19	0.963	0.121	0.904	0.150	0.717	0.176	0.778	0.199
Policy * Age 20	0.876	0.120	0.843	0.131				
Number of Obs	190,423		176,024		161,856		192,476	
Number of Persons	21,337		19,810		18,305		21,070	
B) Time to First-Birth								
	Sample A		Sample B		Sample C		Sample D	
	Odds Ratio	S.E.	Odds Ratio	S.E.	Odds Ratio	S.E.	Odds Ratio	S.E.
Policy * Ages 12-14	0.442*	0.215	0.376	0.242	0.396	0.295	0.495	0.451
Policy * Ages 15-17	0.600***	0.103	0.556***	0.109	0.480***	0.121	0.588*	0.160
Policy * Ages 18-19	0.816	0.111	0.702***	0.087	0.594***	0.065	0.664***	0.095
Policy * Age 20	1.204	0.195	1.002	0.111				
Number of Obs	158,266		146,611		135,167		162,978	
Number of Persons	19,861		18,328		16,816		19,582	
C) Time to First-Birth after Marriage								
	Sample A		Sample B		Sample C		Sample D	
	Odds Ratio	S.E.	Odds Ratio	S.E.	Odds Ratio	S.E.	Odds Ratio	S.E.
Policy * Ages 12-14	1.639	0.634	1.789	1.043	2.029	1.433	2.565	2.061
Policy * Ages 15-17	0.668***	0.089	0.670**	0.115	0.663*	0.158	0.733	0.197
Policy * Ages 18-19	0.757**	0.103	0.676**	0.104	0.610**	0.143	0.643*	0.166
Policy * Age 20	1.137	0.240	0.873	0.095				
Number of Obs	20,706		19,816		18,935		25,186	
Number of Subjects	8,222		7,875		7,516		9,676	

Notes: The data are arranged in a duration analysis format, in which the waiting time concept is age. In panel (a), each woman enters the risk set when she is 10 years old and remains in the sample until she gets married; in panel (b), each woman enters the risk set when she is 12 years old and remains in the sample until she gives birth; in panel (c), a woman enters the risk set when she gets married and remains in the sample until she gives birth. In all panels, right-censoring takes place when the woman completes age 21 or reaches the age at which she is surveyed. A logistic functional form specification is chosen. The baseline hazard function (in age) has a piecewise constant form. The original sample, sample A, includes 35 birth-year cohorts: 1964-1998. In Sample B, 1986 and 1987 birth cohorts are omitted; in sample C, 1985 to 1988 birth cohorts are omitted. Sample D includes 1954 to 1998 birth cohorts, excluding 1985 to 1988. The regressions run for samples A, B, and C include age-varying controls for a linear time trend, whereas the regressions for sample D include age-varying controls for a quadratic time trend. Other controls include mother's mother-tongue (Kurdish, Arabic) and mother's years of schooling--which are both allowed to vary by age (the baseline variable)--and the location of residence of the woman when she was 12-years-old (in the form of 35 dummies for the interaction of the type of location of residence [large city, small city, rural] with 12 NUTS-1 level geographical regions). Standard errors are clustered at the level of year of birth. *** statistical significance at 1 percent level; ** at 5 percent level; * at 10 percent level.

APPENDIX

Figure A1: Assessing Linearity of the Trend in Fraction Ever Married and Fraction Ever Given Birth by Age

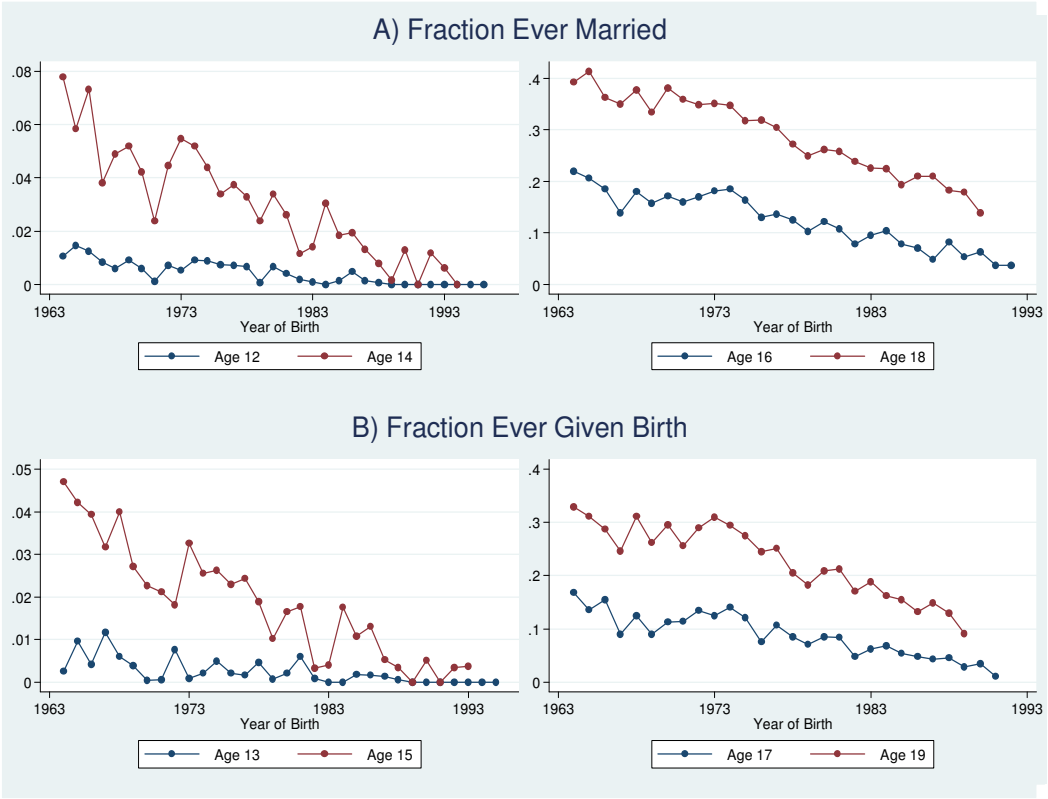


Table A1: Effect of Education Policy on Grade Completion for Women and Men – Quadratic Time Trend

A) Women									
	Sample B			Sample C			Sample D		
Grade	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.
1	0.695	0.197	21,960	0.779	0.240	20,159	0.845	0.222	23,542
2	0.664	0.197	21,960	0.730	0.243	20,159	0.799	0.229	23,542
3	0.632	0.183	21,960	0.638	0.243	20,159	0.721	0.240	23,542
4	0.573	0.220	21,960	0.598	0.258	20,159	0.747	0.289	23,542
5	0.653	0.256	21,138	0.715	0.293	19,337	0.827	0.286	22,720
6	3.796***	1.141	20,372	4.609***	1.439	18,571	5.331***	1.269	21,954
7	3.851***	0.896	19,594	4.658***	1.056	17,793	4.710***	0.843	21,176
8	3.402***	0.704	18,776	3.870***	0.986	16,975	3.970***	0.765	20,358
9	1.565***	0.174	18,027	1.389*	0.269	16,226	1.207	0.184	19,609
10	1.287*	0.175	17,124	1.276	0.259	15,781	1.069	0.164	19,164
11	1.309**	0.175	16,675	1.335	0.267	15,332	1.076	0.164	18,715
B) Men									
	Sample B			Sample C			Sample D		
Grade	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.
1	1.010	0.321	10,453	0.866	0.310	9,799	0.839	0.312	10,838
2	0.664	0.217	10,453	0.519	0.217	9,799	0.553	0.236	10,838
3	0.717	0.244	10,453	0.615	0.288	9,799	0.637	0.305	10,838
4	0.607	0.243	10,453	0.749	0.371	9,799	0.855	0.450	10,838
5	0.546	0.241	10,011	0.490	0.225	9,357	0.626	0.322	10,396
6	2.610***	0.510	9,577	2.853***	0.764	8,923	3.681***	0.863	9,962
7	2.553***	0.534	9,159	2.766***	0.661	8,505	3.572***	0.706	9,544
8	2.593***	0.534	8,738	2.724***	0.600	8,084	3.263***	0.577	9,123
9	1.219	0.257	8,336	1.241	0.266	7,682	1.253	0.250	8,721
10	1.247	0.217	7,943	1.276	0.245	7,289	1.088	0.209	8,328
11	1.188	0.218	7,534	1.315	0.286	6,880	1.068	0.216	7,919

Notes: Sample B and sample C are as defined in Table 3. Sample D includes all 1954 to 1998 birth-cohorts, excluding 1985 to 1988. A separate regression is run at each school year from 1 to 13 where the dependent variable is completion of school year. In addition to the dummy variable for the education policy, the specifications include a control for a quadratic time trend as well as controls for mother's mother-tongue (Kurdish, Arabic), and the location of residence at age 12 (in the form of the type of location of residence [large city, small city, rural] and 5 geographical regions [west, south, central, north, east]). The sample for men is based on the 2008 survey only as the 2003 survey do not include information about the type of location of residence for men, whereas the female samples are based on both 2003 and 2008 surveys. Standard errors are clustered at the level of year of birth. *** statistical significance at 1 percent level; ** at 5 percent level; * at 10 percent level.

Table A2: Effect of the Education Policy around the Discontinuity

A) Dependent Variable: Ever Married							
	A1) 1983-1985 and 1988-1990				A2) 1983-1984 and 1989-1990		
	Odds Ratio	S.E.	No. Obs.		Odds Ratio	S.E.	No. Obs.
Age 12	0.277	0.280	4,762		--	--	--
Age 13	0.195***	0.088	4,762		0.223*	0.171	3,151
Age 14	0.311**	0.145	4,328		0.282	0.230	2,717
Age 15	0.475***	0.078	3,939		0.498**	0.154	2,328
Age 16	0.761**	0.103	3,526		0.627***	0.073	2,328
Age 17	0.824	0.116	3,526		0.669***	0.042	2,328
Age 18	0.790**	0.089	3,526		0.701***	0.092	2,328
Age 19	0.883	0.114	2,648		--	--	--
B) Dependent Variable: Ever Birth							
	B1) 1983-1985 and 1988-1990				B2) 1983-1984 and 1989-1990		
	Odds Ratio	S.E.	No. Obs.		Odds Ratio	S.E.	No. Obs.
Age 13	0.300	0.345	5,202		--	--	--
Age 14	0.299	0.232	4,328		0.294	0.290	2,717
Age 15	0.326*	0.194	3,939		0.283*	0.194	2,328
Age 16	0.429***	0.131	3,902		0.370***	0.138	2,328
Age 17	0.589***	0.113	3,902		0.519***	0.119	2,328
Age 18	0.686**	0.106	3,902		0.599***	0.116	2,328
Age 19	0.618***	0.095	2,925		--	--	--

Notes: A separate regression is run by age where the dependent variable is ever married status in panel (a) and ever given birth status in panel (b). In addition to the dummy variable for the education policy, all specifications include controls for mother's mother-tongue (Kurdish, Arabic), mother's educational attaiment, and the location of residence at age 12 (in the form of the type of location of residence [large city, small city, rural] and 5 geographical regions [west, south, central, north, east]). Standard errors are clustered at the level of year of birth. Some cells for ages 12 and 13 are missing because the policy effect is not identified due to infrequent occurence of marriage and birth at these ages. Some cells at age 19 are missing because the earliest waves of birth cohorts that are affected by the policy are excluded due to the fuzzy nature of the discontinuity. *** statistical significance at 1 percent level; ** at 5 percent level; * at 10 percent level.

Table A3: Robustness Check – Different Year-of-Birth Intervals

A) Dependent Variable: Ever Married									
	Year of Birth > 1953			Year of Birth > 1958			Year of Birth > 1968		
	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.
Age 13	0.178***	0.114	20,154	0.193**	0.125	19,988	0.260*	0.182	15,105
Age 14	0.359**	0.160	19,350	0.379**	0.170	19,184	0.379**	0.187	14,301
Age 15	0.525***	0.113	18,618	0.520***	0.112	18,452	0.520***	0.122	13,569
Age 16	0.637***	0.091	18,214	0.670***	0.097	18,048	0.714**	0.114	13,165
Age 17	0.704***	0.095	17,805	0.761**	0.106	17,639	0.787	0.122	12,756
Age 18	0.794*	0.099	17,401	0.828	0.106	17,235	0.870	0.114	12,352
B) Dependent Variable: Ever Birth									
	Year of Birth > 1953			Year of Birth > 1958			Year of Birth > 1968		
	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.
Age 13	0.150**	0.138	21,765	0.143**	0.131	20,772	0.108**	0.114	15,889
Age 14	0.414	0.261	20,961	0.441	0.283	19,968	0.392	0.264	15,085
Age 15	0.328***	0.102	20,229	0.351***	0.113	19,236	0.354***	0.141	14,353
Age 16	0.460***	0.122	19,412	0.478***	0.129	18,419	0.535**	0.158	13,536
Age 17	0.551***	0.123	19,003	0.569**	0.127	18,010	0.593**	0.142	13,127
Age 18	0.760**	0.092	18,599	0.782**	0.096	17,606	0.843	0.124	12,723
Age 19	0.681***	0.090	17,721	0.689***	0.092	16,728	0.721**	0.100	11,845

Notes: The samples in panel (a) for ever marriage excludes 1985 to 1988 birth cohorts, like "Sample C" in Tables 4, because the predictions for ever marriage in Table 5 are based on this sample; whereas the samples in panel (b) for ever birth excludes 1986 and 1987 birth cohorts, like "Sample B" in Table 4, as the predictions in Table 5 for ever birth is based on this sample. For each sample, a separate regression is run by age where the dependent variable is ever married status in panel (a) and ever given birth status in panel (b). In addition to the dummy variable for the education policy, all specifications include a control for a linear time trend as well as controls for mother's mother-tongue (Kurdish, Arabic), mother's educational attainment, and the location of residence at age 12 (in the form of the type of location of residence [large city, small city, rural] and 5 geographical regions [west, south, central, north, east]). Standard errors are clustered at the level of year of birth. *** statistical significance at 1 percent level; ** at 5 percent level; * at 10 percent level.

Table A4: Effects of Linear Time-Trends on Ever Being Married and Ever Giving Birth by Age

A) Dependent Variable: Ever Married									
	Sample A			Sample B			Sample C		
	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.
Age 11	0.963	0.030	21,634	0.953	0.033	19,999	--	--	--
Age 12	0.935***	0.015	20,882	0.928***	0.016	19,247	--	--	--
Age 13	0.928***	0.010	20,112	0.921***	0.009	18,477	0.922***	0.011	16,866
Age 14	0.949***	0.008	19,308	0.949***	0.009	17,673	0.951***	0.010	16,062
Age 15	0.960***	0.004	18,576	0.962***	0.004	16,941	0.961***	0.005	15,330
Age 16	0.963***	0.004	17,759	0.965***	0.004	16,124	0.968***	0.004	14,926
Age 17	0.964***	0.005	16,889	0.968***	0.004	15,715	0.971***	0.003	14,517
Age 18	0.971***	0.003	16,002	0.971***	0.003	15,311	0.974***	0.003	14,113
Age 19	0.969***	0.003	15,124	0.969***	0.003	14,433	--	--	--
Age 20	0.966***	0.005	14,372	--	--	--	--	--	--
B) Dependent Variable: Ever Birth									
	Sample A			Sample B			Sample C		
	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.	Odds Ratio	S.E.	No. Obs.
Age 12	0.907*	0.046	20,882	--	--	--	--	--	--
Age 13	0.934***	0.024	20,112	0.932**	0.027	18,477	--	--	--
Age 14	0.933***	0.013	19,308	0.930***	0.014	17,673	0.933***	0.016	16,062
Age 15	0.941***	0.010	18,576	0.939***	0.011	16,941	0.938***	0.012	15,330
Age 16	0.948***	0.006	17,759	0.948***	0.007	16,124	0.948***	0.008	14,926
Age 17	0.961***	0.005	16,889	0.964***	0.005	15,715	0.966***	0.006	14,517
Age 18	0.970***	0.004	16,002	0.970***	0.005	15,311	0.974***	0.004	14,113
Age 19	0.973***	0.003	15,124	0.975***	0.003	14,433	--	--	--
Age 20	0.971***	0.003	14,372	--	--	--	--	--	--

Notes: The original sample (sample A) includes 35 birth-year cohorts, 1964-1998, for 10 years old; 30 birth-year cohorts, 1964-1993, for 15 years old; and, 25 birth-year-cohorts, 1964-1988, for 20 years old. Due to the fuzzy nature of the discontinuity, 1986 and 1987 birth cohorts are omitted in Sample B, and 1985 to 1988 birth cohorts are omitted in Sample C. For each sample, a separate regression is run by age where the dependent variable is ever married status in panel (A) and ever given birth status in panel (B). In addition to the dummy variable for the education policy, all specifications include a control for a linear time trend as well as controls for mother's mother-tongue (Kurdish, Arabic), mother's educational attainment, and the location of residence at age 12 (in the form of the type of location of residence [large city, small city, rural] and 5 geographical regions [west, south, central, north, east]). Standard errors are clustered at the level of year of birth. Some cells for age 12 and 13 are missing because the policy effect is not identified due to infrequent occurence of marriage and birth at these ages. Some cells at the latest ages, ages 19 and 20, are missing because the earliest waves of birth cohorts that are affected by the policy are excluded due to the fuzzy nature of the discontinuity. *** statistical significance at 1 percent level; ** at 5 percent level; * at 10 percent level.